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ABSTRACT

Written by a biologist, this paper is intended to present information on the sociological study of man from a biological perspective. Perspectives include that (1) sociology neglects biological variables that are part of understanding human behavior and human societies, (2) the sociobiological or evolutionary view of human development is inadequate to the task of relating biology to the sociological realm and represents a narrow and biased view of human nature; and (3) that the views and approaches of psychologist T.C. Schneirla and his students have been overlooked by sociology and have much to offer the sociologist who seeks a more complete understanding of human social behavior. More specifically, review of the work of Schneirla, a comparative psychologist who devised a theory and methodology for the comparative study of human behavior based on the behavior of army ants, can provide sociologists with understanding of the complementarity of field and experimental lab research, an evolutionary approach to physical and behavioral studies of different species, the application of a comparative method based on process rather than analogy or homology, a sensitivity to the dissimilarities as well as the similarities among species or behaviors, and a conception of a dynamic integrative process. Schneirla's approach can be particularly useful in studying phenomena such as feminism--a subject in which topics including inherent gender differences have been often misinterpreted by sociobiologists. (DB)

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TOWARDS A BIOSOCIAL PERSPECTIVE:

SUGGESTIONS FROM A BIOLOGIST

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I am a biologist by training, a molecular biologist by trade; I have sometimes referred to myself as an "osmotic sociologist" (causing great stirs and whisperings among professional sociologists who are afraid they have missed a new development in the field) because I have for 20 years been married to a sociologist, and have interacted socially primarily with social scientists--who make much better dinner companions than do biologists. I have absorbed--by a kind of osmosis--a good deal of sociological theory and thought; I have even read Mills--though not Parsons, I admit. I also have a 25-year history of political activism, which involves a great deal of brushing up against the "socio" if not the "ology" and does stimulate the sociological imagination. Apologies, then, to any who view me as an interloper--which I undoubtedly am: I do disavow, however, total sociological ignorance and insensitivity.

As an "osmotic sociologist", I have attended most ASA meetings in the past decade, and have even participated on occasion. Most recently, I attended a Roundtable discussion on "Sociobiology" at the 1979 Boston meeting, where I also read Jencks' (1) paper on heredity and environment. These experiences, plus participation in an interdisciplinary Sociobiology seminar, led by UCSB evolutionary biologist Robert Warner (where I, God help us, "represented" the sociological perspective--due to the absence of any "real" sociologists), stimulated a desire to attempt to inform sociologists about some biological perspectives on the sociological study of Man--hence, this essay. The perspectives that I wish to present are: (1) sociology, as a discipline, is guilty of neglecting biological variables that are part of the understanding of human behavior and human societies; (2) the

"sociobiological" or evolutionary view of human development, as expounded by E. O. Wilson (2) or D. Barash (3), and incorporated by P. L. van den Berghe (4,5) and, to some degree, by A. Rossi (6), is inadequate to the task of inserting biology into the sociological realm and represents a narrow, biased view of "human nature"; 3) the views and approaches of psychologist T. C. Schneirla and his students have been overlooked by sociology and have much to offer the sociologist who seeks a more complete understanding of human social behavior.

Discipline Divergence

In recent years, perhaps stimulated by the popular works of R. Ardrey (7,8) and D. Morris (9) as well as by a changing social and political climate (a point I shall return to), or perhaps by the spectacular advances made by molecular biologists in the understanding of the mechanisms of gene action, the halls of sociology have echoed with cries of "return to biology!" and "mea culpa" for the neglect or minimization of the role of biology in the sociological study of human behavior (Rossi (6), Ellis (10), Mazur and Robertson (11), Pratt (12), and, most loudly, van den Berghe (4,5)). The history of the divergence between sociology and biology since the days of Spencer is well documented (6) and justified (6,11) or deplored (5). Ellis (10) and van den Berghe (5) question the legitimacy of sociology as a science at all, and assert the failure of social programs because of the divergence. All call for the reintroduction of biological considerations into sociology.

As a biologist, currently working in molecular genetics, I am struck with the parallels of this historical divergence to a similar divergence in biology--that between eugenics and genetics. In a review of Genetics and American Society (13), a book which documents the misuse of genetics in the development of American social policy in the first third of this century, E. A. Carlson (14) points out that research in genetics as well as events in Germany in the 1930's de-legitimated the earlier eugenic claims. A genetic-eugenic divergence emerged:

> "The pseudoscientific nature and the eventual death of American eugenics were so traumatic that they had a paralyzing effect on the development of human genetics as a legitimate field of research and kept physicians, for more than 40 years, from seriously considering medical genetics as a subject worthy of research or inclusion in the medical curriculum." (p. 585)

Unlike some of the sociologists cited above, Carlson does not deplore this divergence--particularly since the science of human genetics seems well on the road to recovery. Indeed, he ends his review with a plea for scientists to relate their science to values, and to act to influence society's use of their science.

A similar tack is taken by W. B. Provine (15), a historian of science, in summarizing the change in geneticists' minds which took place between 1930 and 1950 with respect to the negative effects of race crossing. He points out that from a condemnation of wide race crosses published by many geneticists before 1930, an agnostic view emerged during the 1930's, culminating

in the belief that wide race crosses were at worst biologically harmless, published during and shortly after World War II. These changes were not based on new compelling data from studies of actual human race crosses; they were, Provine documents, based on the revulsion of educated people to Nazi race doctrines and their use in justifying extermination of Jews. "Geneticists revised their biology to fit their feelings of revulsion." (p. 796). Again, Provine does not condemn geneticists because social and political factors have influenced their scientific conclusions. "It is necessary and natural", he argues, "that changing social attitudes will influence areas of biology where little is known and the conclusions are possibly socially explosive.... Geneticists and the public should realize that the science of genetics is often closely intertwined with social attitudes and political considerations." (p. 796-my emphasis).

The parallel with the biology-in-sociology debate is clear. Sociologists recoiled in horror from the racism, sexism and elitism of the Social Darwinists, rejected biological determinist theories and sought to develop historic and economic models for the design and control of social systems--not necessarily on the basis of "new and compelling data". Similarly, the current trend is toward a somewhat revised version of the earlier models--again in the absence of extraordinary research findings, but rather in areas "where little is known". Here is fertile ground for the growth of "new syntheses", "evolutionary models" and other speculative, all-encompassing theories. In the land of "where little is known", speculation is king, and social and political attitudes, his court.

The solution, then, to the historical consequences of the divergence in sociology, lies in the increasing elucidation of the role of biology in sociological study. Thus, Kunkel (16) calls for research to determine if

biological factors--physiological, chemical or genetic--play a role in human social phenomena and processes; Mazur and Robertson (11) suggest research in areas like the interactive effects of adrenal hormones and the personal and social environment, and Rossi (6) calls for consideration of the biology of mother-child interactions in analyses of the family as a social institution. All of these pleas for data reflect: 1) the paucity of data presently available, 2) an interactionist perspective between organism and environment, rather than a determinist or rigidly reductionist view, 3) a recognition (varying with different authors) that biology does not simply mean evolution or genetics, but includes anatomy, physiology, embryology and biochemistry--all of which play a poorly understood role in human social behavior. It is these perspectives that I share, and it is the need I perceive for their reiteration that prompts this discussion.

Sociobiology in Sociology

Perhaps the most vocal exponent of "back-to-biology" has been P. L. van den Berghe, notably in his ASR article (4) and in his text Man in Society (5). Unfortunately, the biology he calls us back to is the pseudo-biology of Lorenz (17), Morris (8,9) and Tiger and Fox (18): a basically hereditarian, instinct-theory view of man-as-beast, which virtually ignores the most recent work in ethology and primatology (19). Although his work appeared (or was prepared) before the appearance of E. O. Wilson's magnum opus (2) and Wilson scorns the works of Lorenz et al. as "advocacy", it suffers from the same fundamental fallacies that underlie Wilson's theorizing, and which have been discussed in detail elsewhere (20,21,22,23,24). Briefly, these include:

1) Confusing analogy and homology in evolution

2) Rejection of contradicting data

3) Construction of "just-so" stories to account for elements of behavioral evolution--stories which fit the cleverness of the investigator better than they do the data

4) Plethora of untested and untestable assumptions and hypotheses presented as "facts"--including the genetic basis in humans of such traits as xenophobia, religion, ethics, social dominance, slavemaking, indoctrinability, etc.

5) Use of unproven theories of "kin selection" and "reciprocal altruism"

6) Panglossism--"everything that exists is adaptive"--a notion that is rejected by modern neo-Darwinists because of the many examples of non-adaptive linkages and redundancies in DNA sequences

7) Inherent sexist bias (3,24)

But, I suppose, van den Berghe should not be made to suffer for the sins of his "coreligionists", so a brief look at van den Berghe's own assertions in his ASR article is in order. Van den Berghe's thesis is that Man shares territoriality, dominance hierarchy and aggressivity with his primate forbears, and that these elements are basic, constitutional elements of both Man and Primate; they are part of both human nature and chimp nature. But Man is more so (of each trait) than monkey, and particularly more aggressive, because Man is more densely populated and has socially created "insatiable" needs (p. 778), which increase competition for scarce resources, which, in turn, leads to increased levels of aggression in a territorial, hierarchial species--such as Man. The "proof" of this theory rests on the observed universality of these traits in both Man and Primate, the fact that Man is more densely populated than monkeys, Man is the only

cannibalistic, homicidal, war-making animal--a way of dealing with population pressures and concommitant resource limitations. The theory, then, explains aggression more satisfactorily (to van den Berghe) than do the socio-cultural learning theories of Bandura (25).

On his way to this grand bio-social theory of aggression, van den Berghe makes some statements or assumptions which are contradicted by known evidence. For instance, both cannibalism and homicidal warfare are important (to the theory) as means of population control ("Homo sapiens...extensive propensity for cannibalism"--p. 783, "war was (to primitive societies) a quick and effective way to reduce population pressures"--p. 786). Unfortunately, anthropological research (cited in 21) indicates that primitive warfare was rarely lethal to more than a few individuals per episode of warfare, and cannibalism was rare and ritualistic rather than "extensive" or of caloric significance, if it indeed existed at all. Despite this careful anthropological research, most assumptions concerning primitive societies (in the absence of an archeological or paleontological record) are based on evolutionary speculation and analogy with animal social groups--"just-so" stories, with little or no scientific basis. This is particularly evident in van den Berghe's little picture of "mute hominids bashing each other over the head" (p. 784).

Although the plasticity of animal territorial, "aggressive", and hierarchial behavior is well known in the ethological sources cited by van den Berghe (notably Alexander and Crook), he has not heeded the warning sounded by S. T. Emlen--a socio-biology defender:

"The observed plasticity of social behavior should serve as a warning to those who propose overly simplistic genetic explanations

for such behavior patterns as...territorial imperatives in man. Indeed, it is unfortunate that many early popularizers of animal behavior did precisely this."

These popularizers are also van den Berghe's primary sources and he shares with them the continual overlooking of the fact that aggressive or territorial behavior in primates, for instance, is almost as changeable as the weather: it varies with ecology, with presence or absence of predators, with the seasons, etc. In short, it does not even resemble what is called (by the sociobiologists) "genetically fixed" or "species-specific" behavior, and is not considered (by them) a hallmark of "chimp nature".

Another source whom van den Berghe cites, but then seemingly ignores, is John H. Crook, a British biologist-ethologist. In a brilliant article originally appearing in Social Science Information (14(3/4)-89-112) and reprinted in (19), Crook outlines the problems of comparing social organization in animals (between differing species or sub-species) and in human society (between animal and human). He cites his and others' work demonstrating that complex social behavior in animals can only be understood by way of three perspectives: the spatial dispersion of individuals within a given population, processes mediating, promoting and preserving that dispersion, and the dynamic interplay between individuals involved in intergenerational change (i.e., breeding and rearing behavior). In short, it is a sociological or psycho-sociological perspective and method of analysis that is necessary to understand hamadrayas baboon or gelada baboon societies and the differences and similarities between them, rather than the narrow phylogenetic one which forms the basis for the Lorenz-Morris-van den Berghe advocacies. With respect to human societies, Crook argues that Man's unique language capability,

when added to those processes of "proto-cultural" transmission which are crucial even in the development of primate societies, plus the prolonged dependence of human offspring and the socialization this produces, create an important discontinuity between animal and human societies which poses a central problem in any attempted synthesis of primate ethology and social anthropology.

> "Social ethology, if it is to include Man within its scope, needs to examine the implications of Man's uniqueness as well as the evidence for his animal heritage." (19, p. 258)

It is interesting to note that here we have biologist Crook calling for a sociological analysis of baboon behavior, while sociologist van den Berghe calls for a reduction of human behavior--like war--to genetic anlagen. Is this some sort of "discipline-envy"? Or does it perhaps result from a priori assumptions brought to the discipline by each investigator? We tend to find, in nature, what we are looking for, and what we look for is determined by who we are, what we believe, and what use we wish to make of our findings: all this before we even begin to investigate as scientists.

Crook, the biologist, in discussing the writings of Lorenz, Morris and Ardrey recognizes that:

> "The questions these authors raise are concerned with our image of ourselves. This does lead to political issues in the same way as divergent views on the bio-psychology of the two sexes affects thinking in the area Kate Millet calls "sexual politics". The image which we have of ourselves affects our thinking of ourselves. The

> way in which we think of our origin may be crucial in determining what we do with ourselves. Undoubtedly, in the last few years one of the trends has been the development of what I would call a zoomorphic image of Man, and this turns the issue into a political question." (p. 263)

L. Eisenberg, a clinical psychologist, states it this way:

> "I suggest that theory in biology has been influenced by the social role of science, and by the society in which a science exists at any point in time. That is, having come to a particular view of man in nature as a function of one's own social setting, one reads that view into biology as though it arose from independent scientific study. Then one returns to the political arena with <u>apparently</u> independent scientific evidence for the validity of one's preconceptions." (27, p. 520)

Thus, van den Berghe begins with the assumption that sociology is not a science (5) because of its predictive failure, i.e., social policies predicated on sociological conclusions don't "work", looks for alternative paradigms that will produce solutions that do work and concludes with an admonition not to "stick our social scientific heads into the biological sand" (4, p. 788) but to confront the "spectre of eugenics". That is, we must "develop our capacity for controlled biosocial change", presumably through controlled breeding. Despite the obligatory disclaimers ("we are

not slaves to our glands" even though "we are by nature aggressive, territorial and hierarchial", "biology is no more destiny than history is"), van den Berghe does seem to end up calling for biological solutions to social problems.

This is nothing new; in fact, one might argue that it is the inevitable outcome of zoomorphic theories, evident not only in the social Darwinism of the last century but in Lorenz in 1940 calling on the state to insure the "selection for toughness, heroism, social utility", because social institutions had interfered with biological selection and induced "degeneracy".

> "The racial idea as the basis of our state has already accomplished much in this respect. The most effective race-preserving measure is...the greatest support of the natural defenses.... We must--and should--rely on the healthy feelings of our Best and charge them with the selection which will determine the prosperity or the decay of our people...."
> (Z. Angew. Psychol. Charakterkunde 59,2 (1940)--quoted by Eisenberg (28))

The dynamic seems to be: pre-existing ideology contributing to a zoomorphic view of social behavior, leading, in turn, to biological determinism and its concommitant social prescriptions: racism for Lorenz, eugenics for van den Berghe. An example of the possible operation of this dynamic may also be found in the embedding of sexism in sociobiological theory as expounded by Wilson (2), Barash (3) and van den Berghe. If we assume that these gentlemen begin with a sexist bias (no matter how strong or weak), common to all members of this society, it is but a short step to Barash's "Ironically, mother nature appears to be a sexist" (3, p. 283) or Wilson's

"even with identical education and equal access to all professions, men are more likely to play a disproportionate role in political life, business and science" (29) or van den Berghe's "(aggression) is hormonally controlled through testosterone in both humans and in other mammals. Differences in hormonal levels explain why males are consistently more aggressive than females...." (4, p. 782). Then, observations of animal behavior--from this perspective, and within this frame of mind--are made, which develop into a sociobiological evolutionary theory based on the aggressive sexual selection of males and the natural selection of male traits; contradictory evidence (30,31) is ignored, and the status quo emerges as "adaptive" or "biologically programmed". (For a more complete discussion of sexism in sociobiology, see Alper et al. (24).)

The Theoretical Perspective of T. C. Schneirla

To counter this disturbing trend toward biological solutions to social problems, and the racist, eugenic and sexist implications it contains, I would like to reacquaint sociologists with the work of T. C. Schneirla and his students.

Schneirla was a comparative psychologist, one of the founders of that discipline as distinct from the older, mostly European one of animal ethology. His primary animal of study was the Army Ant--an interesting parallel to E. O. Wilson, who is, by trade, an entymologist who also concentrated his studies on the social insects. Gerard Piel (32) has characterized Schneirla's work on the Army Ant as approaching the complete elucidation of the behavioral reportory of this animal. He worked as a comparative psychologist, not simply as an entymologist seeking to describe ant behavior; he sought to understand "the most complex instance of organized mass behavior occurring regularly outside the homesite in any insect, or, for that matter any

infra-human animal" (32, p. 8)--the frequent and periodic migration of the entire colony to a new location. Earlier observers had ascribed a marvelous organizational system, military (hence the name "army ant") and monarchical in nature, involving royal commands, ladies-in-waiting, lieutenants and scouts, even a notation that the "light-colored officers" keep the "common dark-colored workers" in line. Later observers (including E. O. Wilson) attributed the periodic migrations to a decrease in the abundance of food in the foraging areas of the colony--an ecological model similar to many in use today. By careful observations and experimentation, Schneirla demonstrated that the migration episodes were related to the periodic reproductive cycle of the colony, with the stimulatory effects of the mass of wriggling larvae arousing the workers to large raids, ending in emigrations. When the larvae become enclosed as pupae, stimulation of the workers is reduced, the raids are smaller, and do not end in migrations. Simultaneously, the pupation of the larvae reduces their food consumption, permitting more feeding by the queen which produces her next ovulation cycle and the reproductive-migration cycle begins again.

This elucidation of the process whereby complex social behavior takes place derived from and contributed to Schneirla's development of a theory of behavior and a methodology for the comparative study of behavior. His approach, based on concepts of levels of organization and their integration, stressed:

a) the complementarity of field and experimental lab research

b) an evolutionary approach to physical and behavioral studies of different species

c) the application of a comparative method based on process rather than analogy or homology

d) a sensitivity to the dissimilarities as well as the similarities among species or behaviors.

His concept of levels of organization and integration (which should find great favor among beleagured sociologists) includes the precepts that: matter is hierarchially organized in terms of complexity; each level subsumes the preceding level; each level has its own characteristics and requirements for study and interpretation; there is continuity between levels of particular processes (e.g., thermodynamic laws) but on each level the forms and processes are different and have their own laws. Thus, a principle like reciprocal stimulation (or trophyllaxis, as it was known in the insect world) could be usefully applied at the insect as well as the human level to examine the effects of social interaction, but to ascribe military chains-of-command to the army ant or insect colony-like patterns of organization to human society was a confusion of levels, involving anthropomorphism in the former and zoomorphism in the latter.

In responding to both the Lorenzian instinct models and the exaggerated learning claims of the psychologists of the period, Schneirla redefined the terms of the traditional debate, and added the critical concept of the ontogeny of behavior. "Maturation" (which had implied a genetically fixed process) he redefined as "referring to the contributions to development from growth and tissue differentiation, together with their organic and functional trace effects surviving from earlier development"; "experience" (which was consistently confused with "learning") was to be defined "as the contributions to development of the effects of stimulation from all

available sources (external and internal) including their functional trace effects surviving from earlier development. "The developmental contributions of the two complexes, maturation and experience, must be viewed as fused (i.e., as inseparably coalesced) at all stages in the ontogenesis of any organism." (33, p. 269) It seems to me that anything resembling a nature-nurture, heredity-environment, instinct-learning debate still in existence is due to a lack of understanding of Schneirla's formulation, or to blind prejudice. As Piel has stated (32, p. 6): "The gap between genes and behavior is not to be bridged; it is the territory to be explored."

Van den Berghe versus Schneirla

Let us examine how Schneirla's approach, and the research findings of those who have chosen his path in their approach to the study of behavior would respond to van den Berghe's most basic assumptions, which he summarizes as "established facts concerning comparative animal behavior":

1) van den Berghe: "The behavioral repertory of every species is determined in part by a set of biological predispositions which are at least to some extent species-specific."

If by a "species-specific biological predisposition" van den Berghe means that pigs have no wings and therefore cannot fly--we have no disagreement. In fact, I would argue that it is only at that level (pigs can't fly) that "species-specific biological predisposition" has any validity. If, despite the "at least to some extent" hedging, van den Berghe means an innate, genetically-fixed instinct, I would refer to Schneirla's rejection of the innate-learned dichotomy (above) and quote Lehrman: "The use of 'explanatory' categories such as 'innate' and 'genetically fixed' obscures the necessity of investigating developmental processes in order to gain

insight into the actual mechanisms of behavior and their interrelations." (34, p. 20). It is the process of development of the organism, its relationship to both the internal and external environment, at each stage of its maturation that can explain its behavior, rather than the application of a concept like "instinct" or "species-specific biological predisposition".

2) van den Berghe: "Animal behavior is modifiable through learning in response to environmental conditions, and the degree to which behavior is modifiable is a function of the organism's complexity and its intelligence."

Schneirla "responds" directly to this formulation--in an article written 17 years earlier (35). He questions "complexity" as a criterion for determining phyletic psychological standing: the complex eye of the insect is more so than the eye of vertebrates; many specific mechanisms of sensitivity and reaction of "lower" organisms excel those of "higher" organisms. "Intelligence" is an undefined term: does it mean "maze-bright", high IQ score, or what? If it means "capacity-to-learn", then we must ask, "learn what?" Different animals do different things well--with levels of complexity or "intelligence" varying widely among them. The thrust of Schneirla's 1957 article is to point out that <u>all</u> behavior is modifiable through <u>experience</u> (as defined by him above). He suggests degree of "plasticity"--greater versatility of behavior in appropriateness to changing environmental situations--as a possible scale on which to order higher and lower phyla.

3) van den Berghe: "Many forms of behavior (known as 'imprinting' or 'critical period' behavior) are the product of a necessary interplay between biological predispositions and environment, which points to the artificiality of opposing these two sets of behavioral determinants."

In van den Berghe's response to Ellis (10) he welcomes the prospect of being "right for the wrong reasons". He's made it! The sub-clause in his point 3) is incontrovertibly correct, but the main clause is problematical. "Imprinting" or "critical periods" enjoyed great popularity in the 40's and 50's. Subsequent research has demonstrated (36) that the original ideas about them--especially their irreversibility--were wrong. Imprinting applies only to some species (mostly birds) and has been shown to be more plastic than Lorenz thought. Critical periods derive from an imprecise use of an embryological phenomenon and also exhibit great plasticity. Further, van den Berghe's use of those terms, as definitions and/or examples of the forms of behavior produced by an interplay of "biological predispositions" (again) and environment trivializes the behaviors for which he will permit this interplay or the elimination of the "heredity-environment" dichotomy. Schneirla (as we have seen above) pointed out that all behavior involves this interplay, and that the dichotomy is always meaningless.

4) van den Berghe: "Man is not unique in transmitting socially learned behavior, human behavior is not radically discontinuous from that of other species."

Schneirla: "The relations between humans and their surrounding conditions are profoundly different from any other animal, even higher primates, mediated as they are through uniquely human use and creation of symbolic language, complex culture and history." (37) Echoed by Crook: "...man's linguistic ability to refer to objects and their relations using a verbal code of nouns and verbs has had psychological effects that raise man to a distinct grade of psycho-social complexity. These effects are so wide-reaching in their implications that, although thin lines of continuity cross

the boundaries between this and animal grades, any comparison is of little value unless the distinctiveness of the human level is adequately examined." (38, p. 254255) Again, the emphasis is on process: the ontogeny of human, and only human, behavior includes symbolic language. To compare the tradition drift of potato washing in Japanese macaques to the transmission of human culture through history in arguing for Man as just another primate is to confuse levels of organization with a vengeance!

In addition to the four "established facts", van den Berghe's argument in his ASR paper rests on assuming the basic bestiality of Man. Twenty-eight years before the publication of van den Berghe's paper, Schneirla "responded" to it. Amid a welter of articles appearing after World War II seeking to "explain" the bestiality of Man the world had just witnessed, Schneirla wrote an article in the Journal of Abnormal and Social Psychology (39) on "Problems in the Biopsychology of Social Organization":

> "Concepts such as dominance hierarchy tend toward a particularistic static type of thinking about social organization, actually distracting attention from the essential problem of group unity. In studying social organization on any level, a theoretical procedure is desirable which centers around the conception of a dynamic integrative process rather than given characteristics such as dominance which may sometimes be absent. We believe that a more thoroughgoing ontogenetic survey of social behavior in the vertebrates will reveal the prerequisite importance of "trophallactic" (mutual stimulation) processes for group unity...."

That is, where dominance hierarchies in animals, and the aggressive behavior needed to maintain them were seen as the forces regulating group formation and unity, Schneirla suggested that it was the mutual stimulation (trophallactic) processes involved in animal-to-animal interactions that formed the basis of the group unity. Dominance and aggression are, in fact, detrimental to group cohesiveness. Concepts of territoriality, dominance and aggression are not required for an understanding of the processes whereby groups form and social behavior develops in both animals and humans, and they are therefore unnecessary for explanations of aggressive behavior. "No analogy with lower animals can adequately explain (Man's) failure to use the maximal psychological facilities at his disposal to settle social difficulties by means other than physical combat." (37, p. 73)

Feminism, Anti-Feminism and Biology

It is, then, the "conception of a dynamic integrative process" that Schneirla can offer sociologists--or offer them the notion that they must search for it in attempting to understand human social behavior. Such concepts must involve the internal environment of the human organism, its relation to the external environment, how the environment impinges on the organism, and what factors-social, psychological and physiological--mediate the processes of interaction. This inter-actionist, integrative perspective is often missing both from Rossi's "A Bio-Social Perspective on Parenting" (6) and from the feminist response of Breines et al. (40), although the underlying political motivations of these authors may be assumed to be quite different from those of Lorenz and van den Berghe, and closer to those of Schneirla, who was a founder of the Society for the Psychological Study of Social Issues (SPSSI).

In an attempt to justify what seems like a heretical position in (at least, feminist) sociology--namely, that men and women are biologically different, and social institutions ought to reflect that difference--Rossi uses the popular, current and fashionable sociobiological assumptions to assert the role of biology. Thus, gender differences in qualities and traits like spatial perception, manual dexterity, emotional stamina, care of newborns and preparation of food are attributed to natural selection; the "universality" of females nurturing newborns in all human society is adjudged proof of "innate, unlearned" female behavior; "propensity for particular social behavior which goes with a given physiology" is lodged in the DNA genetic code; postulation of "innate" factors in parenting behavior is made.

Rossi assumes a Lorenzian "instinct" model of some sort in discussing female behavior toward newborns, although updating it somewhat by referring to recent findings in the neuroendocrinological relations to behavior. All females (human and primate), it is claimed, who give birth to young, are "programmed" by DNA and hormones to form a mother-offspring bond--one which fathers are irrelevant to. Why is it then, that rhesus monkeys raised without contact with their mothers and in extreme social isolation, exhibit bizarre, non-nurturing behavior toward <u>their</u> offspring, often ignoring them and letting them die? (41) Hormones are undoubtedly important, but <u>clearly</u> females who have not given birth or even been pregnant can exhibit maternal behavior--even rats (42); also, fathering behavior is important in many species--without too many female hormones. If it is greater levels of manual dexterity exhibited by human females (due, in part, to the lower muscle-bone ratios in the hands of human females) that account for a "propensity" to child-care in women (for which, Rossi argues, men would need

compensatory education) how do fingerless mammals manage? Or was the human female selected for adaptability to the safety pin? (Not a facetious question in the fantasy world of sociobiology.)

The notion of complex behavior like parenting, being "encoded in the DNA" makes no biological sense. What is encoded in the DNA is a set of templates for the regulated production of various enzymes, proteins and RNA molecules; how this translates to social behavior is not at all clear, and is an important and legitimate area for investigation, but hardly susceptible to conclusive statements as yet. Various experimental manipulations of animals, which in no way affect their DNA, can produce widely differing effects on maternal behavior (43). Numerous studies in birds (44), rodents (43), cats, and monkeys (45) have shown that maternal behavior defies dichotomization into categories such as "innate" and "acquired"--but Rossi does indeed state the "innate" perspective. The evolutionary argument she makes also assumes--as it must--a plethora of innate responses. Theories of evolution of social behavior by natural selection assume that the process of selection operates at the level of the gene--and we're back to "encoding" parenting in the DNA. Rossi uses sociobiological speculations about the evolution of the mother-infant bond to buttress her argument that present day (and future projected) family structures are or may be inimical to the "basic, biological" nature of that bond. Schneirla has pointed out (46) "that no factual justification (exists) for the conclusion that the adaptiveness of... behavior accounts in any way for the events in individual behavior through which it appears." (emph. added) The facts that are needed to justify Rossi's hypothesis lie, not in antiquity (or in other diverse cultures), but in an examination of mother-infant interactions, adding a sociological or social psychological

perspective to what is known, for instance, about the effects of the hormone oxytocin on the mother's milk production. For instance, does this hormone affect or concentration vary with whether a mother works before and after the birth of her child, with whether the birth is strongly anticipated or dreaded? Does a demanding, "colicky" infant produce different hormone levels than a quiescent, placid one? Do birth centers or at-home births produce children who exhibit different patterns of development than hospital-born children? Is the post-natal pattern of family interaction different? Can any long-term effects on children as they mature to adulthood and parenthood be measured which vary with "strength" of early "bonding"? The work on maternal behavior of Harlow et al. on rhesus monkeys (41), Moltz on rodents (43), Rosenblum on Macaques (45), Lehrman et al. on ring doves (44) and Schneirla et al. on cats (47) produce a million more research ideas and hypotheses and bear examination by anyone concerned with the nature of the mother-child bond. The theme in these (and any Schneirla-esque) studies is to explore the interaction of the mother's internal environment with her external environment (which includes the offspring) as well as the interaction of the infant's internal environment (and subsequent development) with its external environment (which includes the mother). Speculations on "innate" or "learned" or "evolved" behavior become, therefore, superfluous. Rossi's sociobiological arguments are both false and unnecessary to the perspective she is trying to enunciate: that the "traditional sociological approach to the family ought to be supplemented by a direct examination of mating and parenting as behavior systems...drawing on the biological, clinical or benavioral sciences." "How does the behavior develop" is the question, and the question of "Why?" becomes merely teleogical.

The (predictable) response from feminists Breines et al. is that Rossi has fallen into the trap of biological determinism and that "social scientists generally and feminists in particular...have made every effort only to resort to biological explanations when no cultural or social structural explanations for social phenomena can be found." (40, p. 43; emph. added) They are, however, guilty of that same biologically determinist bias in implying that biological bases of behavior exist at some deeper, more basic level, which is "resorted to" when sociological levels of explanation have been exhausted.

Tobach (48), in summarizing Schneirla's concept of levels of integration presents an example that should be useful to Rossi, Breines and others interested in a biosocial analysis: knowing that changes in concentration of circulating hormones in fish change the firing frequency of a set of motor neurones (physiological level) does not enable prediction of whether a fish will swim towards or away from another fish (behavioral level); knowing the ontogenetic history of the two fish-the history of the circumstances in which they are now observed, e.g., whether they were raised in isolation or with each other or with other fish--enables us to predict the probability of events on the behavioral level, without necessarily referring to the subsumed physiological level. Causal explanations on any one level require the integration of many levels.

Breines et al. do call for an "interactionist influence model, which allows for the reciprocal implications of social and psychological factors and biological ones", but they make no attempt to present one. Instead, they relegate biology to another level, extrinsic to any model ("resorting to biological explanations"). Attacking Rossi for what she did a little bit

of, but not responding to her call to take female biology into account when examining a social institution like the family--the analysis of which has for so long been through male-oriented paradigms--is not the best kind of feminist thinking.

Towards Biosociology

Is there a course sociologists can follow, which will provide a way out between the Scylla of biological determinism and the Charybdis of biological ignorance? Can there be a "bio-sociology"? I believe that the perspectives of Schneirla and his students chart that course (although, Schneirla, being a psychologist, called for a "psycho-social" analysis) and that sociologists would benefit from a careful reading of their work. In thinking about how I, as a molecular biologist, could communicate Schneirla's perspectives to sociologists, I considered analogies with molecular biology. (I even considered calling for a "molecular sociology", but what with macro- and micro-sociology and sociobiology and biosociology, I realized confusion would be immeasurably increased.) Molecular biology is the study of how biological events occur on a molecular level--i.e., how do changes in the DNA structure produce molecular events that, say, confer resistance to antibiotics in a bacterium. An explanation is not considered complete until each step in this process is understood--and it is not simply a matter of reducing everything to the biochemical level; biochemistry has its own rules and laws--which must be subsumed at the molecular level, of course--but the organism, even a bacterium, must be examined and understood in its own terms. (The early molecular biologists had the devil's own time trying to refute changes of biochemical reductionism--but they did, and established a separate discipline.) It is that way of thinking that T. C. Schneirla presented in comparative psychology, and which needs

re-introduction in "biosociology". A story reported by Michael Chance at a conference (19) perhaps presents it best--as stories often do:

Two saints were crossing a flooded river by walking on the water when a pilgrim arrived at the shore and asked them: "How can you walk on the water? Why don't you sink?" "It is faith that enables us to walk upon the water," answered the first saint, and when the pilgrim hesitated, the second saint added: "Just be strong in your faith and follow us." The pilgrim prayed, looked toward heaven, set his feet upon the water and promptly disappeared beneath the waves. The first saint turned to the other and asked, "Should we have told him about the stepping stones?"

It is those stepping stones we must be searching for: leaps of faith will not do.

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